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The research interests of developmental and educational psychologists concerned with children's learning appear to be converging. Simultaneously, the study of children's learning is at a significant crossroads. In the last decade, research has progressed rapidly and has moved from a problem to a variable orientation. If there is to be a significant impact on teaching practices and on the understanding of children's learning abilities, greater effort must be expended in designing studies that tap the unique characteristics of the child. Examples of characteristics that impose limiting conditions on the application of existing learning theories are discussed. Individual differences in children clearly lead to different approaches to learning, and need to be studied further. A recent experiment in paired-associates learning indicated important differences according to sex. The ways in which incentives influence learning at different levels is another area for research. The role of instructions or verbalizations in children's learning is not fully understood. Other areas of needed research are also suggested. (KP)

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CHILDREN'S LEARNING: CROSSROAD OF DEVELOPMENTAL AND EDUCATIONAL PSYCHOLOGY

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I have spent a great deal of time during the past year reviewing the literature on children's learning. In the course of reading this research two things continued to impress me, and it is about these that I have organized this talk. First, I have been impressed by the fact that the research interests of developmental and educational psychologists concerned with children's learning appear to be converging. For many decades developmental psychologists were following one route; educational psychologists, another--each pursuing their own well-defined paths and each looking at quite different aspects of the scene. Now, they seem to find themselves interested in many of the same things. They find themselves, in 1968, at the same crossroads. A "crossroads" also may imply that one has reached a point where a decision must be made about where to go next. The second point that continued to impress me was that the study of children's learning is, itself, at a significant crossroads. As I wish to point out, ideas about learning derived from the study of learning in animals and human adults are proving to be less and less fruitful in guiding our discussions about children's learning. We have reached a point where we must decide whether to continue to pursue these ideas or whether we are willing to develop new approaches based on our knowledge about how children learn.

It is impossible now to guess the affiliations of an individual by what he is doing. It should not surprise anyone now that developmental psychologists may be pursuing the study of reading and educational psychologists may find themselves looking at syntactical mediation in paired-associate learning.

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In fact, one of the goals of both disciplines is to increase our knowledge about how children learn and how learning may be made more efficient. At the same time, anyone who has been around for many years may find the present state of affairs rather startling, Cronbach, for example, described the years between 1940 and 1954 as ones in which:

"....commerce between academic psychology and educational psychology was cut off by a tacit embargo. Persons concerned with education found no nourishment in the . . . studies that began to dominate experimental psychology. Experimental psychologists were repelled by the educators' insistence on talking about the 'whole child' in 'real-life situations' -- both being prescientific or even anti-scientific phrases antithetical to the analytic, formal style that, for a time, was the ideal of American behavior theory" (Cronbach, 1965, p.110).

What happened in the study of children's learning during the mid-fifties that led to the repeal of this embargo?

Early research on children's learning was, for the most part, a derivative of psychological studies of learning in animals and human adults. Although the study of children's learning presumably is related to practical concerns in the education and rearing of children, the field initially was dominated by the methods and problems of the experimental psychologist. The laboratory, and not the home or the school, was the locus of research.

The early research included an array of studies on diverse topics whose only common feature was an interest in contrasting the performance of children with that of lower animals or human adults. These studies, and indeed most of the early studies of children's learning were problem oriented. They were concerned with such questions as whether children performed more effectively than lower animals, whether differences in CA and IQ resulted in differences in performance, and how the performance of children differed from that of adults.

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The empirical orientation of such studies might be expected, for theoretical positions of some complexity did not begin to be formulated in discussions of the psychology of learning until the 1930's.

What was the cumulative impact of this research with children, other than replicating the findings of earlier studies with animals and human adults? Surprisingly little. In 1954 Munn attempted to summarize these studies for his chapter on children's learning for the second edition of the Manual of Child Psychology. He concluded his chapter in the following way:

So far as discovering anything fundamentally new concerning the learning process, the investigations on learning in children have failed. One possible reason for this is that such investigations have from the first been patterned too much after the lines of earlier research with animals and adults in the laboratory. A more likely reason, however, is that the phenomenon of learning is fundamentally the same whether studied in animal, child, or adult (Munn, 1954, p. 449).

While one may wish to qualify the second interpretation, Munn's general conclusion seems appropriate. Educational and childrearing practices were not strongly influenced by the work that had been done with children, partly because of the type of research and pattly because there simply wasn't enough of it. This does not mean that teachers and parents were not influenced by psychological research in learning. But the influence came from studies with rats and college sophomores, not from studies with children. The attempts to apply findings from the inarticulate rat and the overarticulate sophomore to the young child was responded to with disgust by many educatiors, but many educational psychologists still taught their courses using the results of studies with mazes, puzzle boxes, and memory drums as examples of the operation of the learning process.

By the early 1950's came the era of learning theories--Hull, Tolman, Skinner, and all the rest. Everyone began to read Hilgard and to learn about

excitatory potential, cognitive maps, and VI schedules. Because of these theoretical advances, research with children in the 1950's became variable oriented, in contrast to the problem orientation of the earlier years. Questions commonly were asked about how experimental variables influence learning, either singly or in interaction, rather than questions about how long it takes a child to learn a certain task, or whether the performance of children differs according to sex, intelligence, and age, or how the performance of children differs from that of lower animals. The impact of theories of learning can be seen in the rapid increase in the rate of publication of studies with children. Between 1940 and 1949 fewer than 100 studies on children's learning appeared in English. Between 1950 and 1959 the number rose to over 200. During the '50's practically all of the studies were reported by developmental psychologists. The early enthusiasm generated by Dewey, Judd, and Thorndike had waned, and educational psychology, at least in the Western countries, became concerned with measurement, not learning. Assessing and predicting academic achievement were carried to sophisticated levels, while interest in the study of learning decreased.

This brings us to the present decade. The pace of research since 1960 has been momentous. Between 1960 and the end of 1967 over 900 articles on children's learning were published in English--roughly the number that had appeared in all the six preceding decades. The Journal of Educational Psychology is flourishing; Child Development and the Journal of Experimental Child Psychology publish large quantities of research on children's learning; and now a new journal, Developmental Psychology, is about to be launched. And the quality of research is improving.

Why now? Why has this been the period in which children's learning has become the focus of so much attention? Was it because there was more money available, better equipment, faster computers? Or was it because researchers

finally focussed their attention on children rather than on isolated problems? Although many factors were involved, I believe the critical step occurred when investigators began to select children for studies of learning, not because of general curiosity, or because they were readily available and cheaper to maintain than the laboratory rat. But when they began to study children because of their own distinctive characteristics--their growing facility with language, their acquisition of social competence, their expanding systems of conceptualization, their greater capacity for attentiveness, and all the rest. Without a theoretical base, this research would have devolved into normative studies. With a theoretical orientation, the research has attempted to integrate, with increasing analytic precision, the role of a multitude of variables, including developmental level, into their discussions of the learning process.

A good example of what I am talking about was the study by Margaret Kuenne Harlow on transposition (Kuenne, 1946). This study, it seems to me, is an important furerunner of the current research, anticipating in 1946 what would be characteristic of the field in the 1960's. The study was theoretically based and developmentally oriented. Dr. Harlow wished to know whether the acquisition of language would influence the learning and transfer of a discrimination. More precisely, she wished to know whether preverbal children would perform in a manner comparable to that proposed by Spence's theory of animal learning, and whether older children, beginning to use words as a means of guiding response, would perform in line with mediation theory. The study excited interest because it was a lucid example of how theoretically based research could be conducted with children, and because it demonstrated how developmental studies might provide a means of bridging the chasm that had separated research with animals from that conducted with mature human subjects. This kind of research required a concern with developmental processes, familiarity with theory, and a willingness to venture forth with new theoretical ideas that might differ from those developed

in accounting for the behavior of lower animals or human adults.

We have no adequate theories of children's learning. All of the present theories prove to be capable of handling only a small portion of the information that has been produced. What may seem at one time to be an effective interpretation proves, upon further investigation, to be inadequate. This would be depressing, except for the fact that our knowledge has enabled us to ask questions of increasing sophistication. As our questions get better, and they surely are, we are approaching more and more rapidly the possibility of developing the first adequate theories of how children learn.

We are at a crossroads. We have reached this point in large part because of a willingness to capitalize on the advances in the general field of learning. We can continue to borrow methods, concepts, and theories, or we can begin to develop our own. If we continue to follow the lines that have been laid out by learning theorists dealing with other types of subjects, we will, in my estimation, severely limit the contributions of further research with children. We will have the illusion of producing significant research while in reality falling in the shadows of those whose goals turn out to be quite different.

The learning process undergoes developmental changes. Learning differs in children at different ages, and it is different in the child and in the adult. Since none of the theorists has talked about children we should not expect to be able to go much further using these theorists as guides. It is somewhat paradoxical that to avoid being replicators and translators we are forced to disengage ourselves from our associations with the general field of learning, especially since the association have been so helpful in getting us to our present level of development. All of us who are interested in the study of children's learning, regardless of our affiliation, must seek new ways of exploring the topic of our interest. We have passed through problem-oriented to variable-oriented research. Now we must pass to child-oriented research. Our theories

must be built around the child. We cannot expect those who never study children to be sensitive to the types of variables that may have great effects on children's performance, while exerting little effect on the performance of rats. This sounds as if we are being thrust into limbo. Fortunately, this is not the case. Research of the past couple years has yielded a number of interesting leads that, if pursued, would seem to be able to provide us with the kinds of information we will need to build developmental theories of learning. I would like to turn now to some of these that seem to be the most promising.

Individual Differences. The last thing a lot of people want to talk about is individual differences in learning. But they exist. The tendency to relegate individual differences to error variance has reduced the significance of many main effects, and not just because they provided too large a denominator for a lot of F tests. Experimental psychologists never have been very much interested in individual differences. Robert Glaser, in his chapter on learning and individual differences for the Gagne volume, writes:

> It is well documented that the German and English traditions of the nineteenth century gave rise to two apparently separate disciplines of scientific psychology represented by the correlationist psychometricians and the experimentalist 'psychonomes.' While major learning theorists have indicated their concern with individual differences, this concern, for the most part, has never risen above the threshold of serious action (Glaser, 1965, p. 1).

Whether individual differences play a more decisive role in children's learning or whether their effects are easier to assess, recent work has demonstrated the ubiquity of their influence. There are many examples. Individual differences in rate of neonatal conditioning were found by Papousek (1967), some infants acquiring a stable conditioned response very rapidly and some requiring more than a month of training. Eisenberg, Coursin, and Rupp (1966) found wide individual differences in neonatal habituation to acoustical patterns.

Going to older children, various training studies dealing with the acquisition of concepts such as those of conservation commonly have found that half or somewhat more of the children benefitted by the training, while the remainder did not. In many of the more traditional learning problems individual differences have been found to be a factor of central importance. I will mention two examples: transposition and reversal-nonreversal shifts.

Despite the stimulation provided by Dr. Harlow's study, subsequent studies of transposition have shown that her theoretical interpretation is only partially effective. But so are all of the other interpretations that have been offered--relational, absolute, discriminability, ratio, and mediation theories. Partial support for each can be found, but none provides an adequate and comprehensive basis for interpreting the data we have available. Could it be that different children learn different things about the stimuli during the training trials, and our earlier efforts to separate children by age or possession of the concept of relative size give us an incomplete description of the differences among children that vitally influence how and what they will learn?

To determine the kinds of differences in learning and transfer that exist, Zeiler and Salten (1967) studied 20 five- and six-year-olds. The children were presented a standard intermediate size problem, and gradients of transposition were plotted for each child. The gradients were of four distinct types. Six children showed uniform transposition across all sets of test stimuli; seven showed predominantly absolute choices; three showed decreasing gradients as the test stimuli became more remote; and four showed inconsistent choices. The authors had no explanation of why the children appeared to be controlled by different aspects of the stimuli, but obviously lumping these data together in groups means would provide an unrepresentative picture of the children's performance. And any attempt to align the group means with any one of the present theoretical positions would appear to be illogical.

We cannot conclude that individual differences among children preclude theoretical analyses of their learning, but we must assume that individual differences among children impose limiting conditions upon the applicability of any of the existing theories. If we knew more about the characteristics of children who tend to make each type of response we would be in a much better position to develop an adequate theory.

The story is a bit different in studies of reversal and nonreversal shifts. These have been popular problems since the Kendlers reported a significant interaction between developmental level and ease of solving each type of problem. For a while it appeared that the stimulus-response-mediational theory proposed by the Kendlers offered a powerful means of interpreting this interaction. But again, as more research was reported, the inadequacies of their position became apparent. For example, investigators began to discover that whether children showed faster reversal or nonreversal learning may depend on differences in cue preferences rather than upon their ability to mediate. If the child's preferred dimension, whether it was size, color, pattern, brightness, or height, happened to be reinforced during the initial problem, both it and the reversal shift were learned more rapidly than if a nonpreferred dimension was reinforced. For example, Tighe and Tighe (1966) tested three- and four-year-olds with stimuli that differed in height and pattern. Children for whom height was the relevant dimension learned the initial discrimination in an average of 4.3 trials, while those for whom pattern was correct required 73 trials. One hundred percent of the children trained with height showed reversal shifts in the optional shift procedure while only 12 percent of those trained with patterns did so. Similarly, Smiley and Weir (1966), who pretested children for their preferred dimension, color, or form, found that 80 percent of the children for whom the preferred dimension was correct on the training problem showed reversal shifts,

while only 20 percent of those whose nonpreferred dimension had been correct did so. How can we hope to understand children's performance on these types of transfer problems if we avoid including the variable of cue preference in our discussion and design?

We have often thought of individual differences as contributing to variability. Clearly, they do this, but more importantly, they, in many instances, appear to lead to quite different approaches to learning. It seems to me that further research on children's learning must include a closer scrutiny of individual differences among children, what the antecedents of these differences might be, and how these differences influence the learning process.

Sex Differences. A second example of a lead worth pursuing is a sub-category under individual differences. It is the spectre of sex differences--another variable many would like to ignore. Sex differences would not be a crucial variable if they led only to differences in average level of attainment. We can look at the better performance of girls than of boys in paired-associate learning and in incidental learning with interest, but the difference of a few correct responses does not require us to reconsider the role of sex in learning, for we merely would assume that the rate of acquisition differed but the learning process remained the same. Other data, however, jar us from our complacency. I would like to give you a few examples from a study we completed recently at the University of Minnesota (Stevenson, et al., 1968).

We were interested, in part, in the relation of laboratory tests of learning and long-term tests of learning. So we correlated performance on such tasks as paired-associate learning, discrimination learning, and others with children's grades in school. Experimentalists often are accused of doing research that has no relation to meaningful learning, so we looked at these correlations with special interest. Let us take paired-associate learning as an example. In

paired-associate learning involving nonsense syllables and abstract words, the correlation for 7th grade boys between the number of correct responses and school grades for one quarter in English was .59; in social studies, .48; science, .74; and math, .67. All substantial correlations and a nice answer to would-be critics. For girls the comparable correlations were .33, .51, .52, and .29, respectively.. Somewhat lower, the first three are significant and still fairly impressive when one realizes we were correlating twenty-minute samples of behavior with learning occurring over 10 weeks. One response to these data is that both forms of learning are determined by intelligence, anyhow, so what is so impressive about these figures. So we recomputed the correlations with verbal IQ partialled out. Now, the correlations for boys were, respectively, .45, .34, .65, and .60--for boys, the correlations were not greatly reduced by eliminating the contribution of differences in IQ. For girls, however, the effect was dramatic. None of the correlations now was significant. Intellectual level had a much stronger influence on the relation between the rate with which girls learned paired-associates and learned in school than it did for boys. As one might expect, the correlations of IQ with rate of learning and with school grades were higher for girls than for boys.

We took the analysis one step further. Since we had separated the data by trial blocks, we were able to correlate children's performance on the first trial block in paired-associate learning, a less-than-three minute sample of behavior, with their school grades. For boys, the correlations were all significant--varying from .32 to .47. For girls, none of the correlations was significant. The fact that the correlations for boys were significant for such a brief sample of behavior indicates that their initial responses in the learning of paired-associates are a good indication of their success in learning over much longer periods of time. Girls apparently approached the paired-associates task differently, and several presentations of the list were required before their performance bore any relation to their long-term learning. Interestingly,

nonintellectual characteristics were correlated with boys' paired-associate learning, but not with girls'. For example, teachers' rating of characteristics such as "Hardworking" and "Enthusiastic about School" were significantly correlated with the performance of boys, with r's of .45 and .43, while in neither case were the correlations significant for girls.

These are only preliminary data, and we do not yet really understand them, but they are fascinating examples of how different variables may influence the learning of girls and boys--intellectual characteristics on the one hand and such factors as diligance and application on the other. How many significant interactions with sex would have been produced in the literature if sex of child had been included as one of the main effects in the analysis of the data?

Incentives. Let us turn now to an entirely different topic, but another interesting example of how current research is leading us away from old notions. One of the carryovers from animal psychology was the strong emphasis on the functions of incentives in learning. Increase the value of the incentive and you will increase the level of performance. But children in many ways are uncharitable subjects, for they often fail to behave in the manner predicted by learning theories or, in some cases, even by common sense. The vagaries of their behavior are strikingly evident in some of the studies of reinforcement effects in discrimination learning. A good example is the study by Miller and Estes (1961) in which nine-year-old children received either a signal light, 1 cent or 50 cents for each correct response in a two-choice discrimination task. The subjects given a monetary reward performed significantly more poorly than the control group, and the group receiving 50 cents for correct response performed no better than the group receiving one cent. If these were isolated findings they would be less compelling, but comparable results have been found in a number of studies. Before we settle down too quickly to thinking about

possible interpretations we must turn our attention to another group of studies dealing with the effects of level of incentive on discrimination learning. In these studies, one response was not reinforced consistently--they are studies of probability learning. In these studies level of incentive has been found to have significant effects on level of response. Siegel and Andrews, for example, found that preschool children reached a lower asymptotic level of response when the rewards were of low value than when they were of high value--results that are more in line with our expectations. The contrast between these two sets of results is tantalizing. In discrimination learning, differences in performance as a function of level of incentive are difficult to obtain; in probability learning differences are easy to find. But to make matters even more complicated, the effects of level of incentive in probability learning may depend upon the subjects' developmental level. In several studies, for example, a low level of incentive has been found to produce a higher level of performance than a high level of incentive with four-year-olds, but with nine- and 14-year-olds, high levels of incentive produced higher levels of performance.

These are not the kinds of results you find reported in the literature on learning in animals or human adults--and researchers in these areas may be glad of that--but they offer good examples of the complexities encountered when one discusses the role of incentives in children's learning--a variable of central importance in any theoretical analysis of the learning process. To be able to handle these data satisfactorily a theory must be capable of predicting when different incentives will and will not influence performance, and how the effects will interact with developmental level. We are always faced with the fact that we are dealing with a developing organism, and theories that may be effective in interpreting the data obtained from children at one developmental level may be ineffective at other developmental levels.

Some at this point may wish to throw up their hands in despair. The problems are complex, but the data show strong regularities. We must assume that a conceptual system can be developed that is not too awesome, and that if we continue our investigations the regularities will become so persistent that the underlying mechanisms will become increasingly clear.

Instructions. And now a final example of a lead we may find productive--the effects of instructions on learning and performance. In teaching children we rely heavily on the power of words as transmitters of information and guides to action. It is assumed by many teachers and by many learning psychologists that, once the child acquires language, he responds to and uses words in the same fashion as other stimuli. Many mediational theorists, for example, assume that the verbal child is capable of using words rather than external stimuli as directors of his responses.

We had some forwarning from the work of Luria that these assumptions might be an oversimplification of what actually takes place. Luria and his associates conducted a series of developmental studies on the role of speech in directing the behavior of young children, and he has summarized his research in the following way:

> It would appear as if the power to respond to speech as a stimulus does not develop until the beginning of the second year: If we say to a child of one and a half years "hold out our hand" or "clap your hands" it is easy to obtain the appropriate response. However, careful observation will show that at this stage the effect of speech is still very limited, and if there is any conflict with some act the child may have started already, the order may be quite ineffectual. We may tell a child who is putting on a stocking to take it off, or one who is taking rings off a stick to put them back, and it will be seen that the intended effect on the original act is not produced, but that on the contrary the action

proceeds more vigorously. At this stage of development, a child's actions are dominant and adult speech has merely a release function; it is not able to suppress an act already begun and still less to deflect a child from one task to another (Luria, 1961, p. 166).

But Luria was talking about studies with very young children whose linguistic facilities are still immature. The young child's difficulties in responding to instructions may have little relevance for discussing the behavior of older children. Let us look, then, at the results of some studies involving older children, where instructions may be more likely to have stable controlling influences on behavior.

I would like to give several examples. The first is a study by Weir on probability learning, where choices of one of three stimuli led to partial reinforcement and choices of the other two stimuli never were reinforced. In such a task the maximum frequency of reinforcement can be obtained by sticking to the reinforcing stimulus, even though it never yields consistent reinforcement. Older children do not do this, but continue to vary their responses, apparently because they enter the task with a strong expectation that consistent reinforcement is available. One should be able, therefore, to instruct them, prior to the task, that there is or is not a method whereby consistent reinforcement is possible. If the instructions are effective, the children should abandon their hypotheses and settle on the response that yields inconsistent, but maximal reinforcement.

Weir (1962) therefore instructed his subjects either that there was a way of obtaining a reward on every trial, or that there was not a way, or gave no instructions. The subjects were 5-, 7-, and 9-year-olds. The youngest children performed at a higher level than the older subjects, as had been the case in earlier studies, but instructions did not have a significant effect on the children's performance at any age. A subsequent study by Gruen and Weir (1964)

included a broader sampling of ages. Now the subjects were 7- and 8-year-olds, 12- to 14-year-olds, and college students. The instructional conditions were the same as those of the previous study. Even with children of these ages instructions had no significant effect on performance. There was an interaction between age and instruction; only college students performed in the manner predicted by showing a higher level of response when they were told that no solution was possible. Apparently, it is very difficult to disengage children from the sets with which they approach the task by verbal instruction. Even 12- and 14-year-olds, children whose language is highly developed, were unable to benefit by the experimenter's comments.

And now let us look at a different, and final example of the role of instructions--in this case in the reversal-nonreversal shift problems. A basic hypothesis in research on these problems has been that if the child verbalizes the dimension of the stimuli that is relevant for response, say size, he will be able to switch rapidly from one value of this dimension to another when the problem changes. For example, if he has been responding to the "large" stimulus he will be able to switch to the "small" stimulus with greater rapidity and thereby show faster reversal learning than will a child who is responding without language to the absolute characteristics of the stimuli. Tracy Kendler, in a study with kindergarten children, told her subjects the basis for correct response and instructed them to verbalize it prior to making each choice during the training trials of the optional shift problem. The children did verbalize, but there was an unsystematic relation between verbalization and performance. The frequency of reversal shifts was increased by instructing the children to verbalize the basis of their response as was predicted. But during the second phase of the task many children continued to make the previously appropriate statements while making the opposite choice from that contained in their verbalizations. For example, " . . . a child who made no adjustment

in the words he spoke would be saying 'Black is the winner and white is the loser' while he consistently chose the white square." (Kendler, 1964, p. 431).

Perhaps the most important conclusion that can be drawn from this and other studies involving instructions to verbalize is that verbal mediation is not sufficient to explain the developmental changes in performance on such tasks as the reversal-nonreversal shift problem. The ability to generate verbal solutions involves more than possessing relevant words. Verbalization is not the beginning, but it may be the end-product of a long series of cognitive activities involved in the solution of the problem. The child may or may not code his activities in terms of words, and in the end, may or may not be able to describe in words what he has done. The evidence seems to undermine analyses of learning and problem solving that consider words simply as stimuli mediating between the perception of the stimulus and overt response. Whenever supplying verbal labels to young children has had a facilitating effect on learning and transfer, the results can be explained parsimoniously by assuming that the labels were effective in directing the child's attention to the relevant aspects of the stimuli. This is not to say that language does not influence the performance of children; it does imply, however, that the mechanistic consideration of words simply as stimuli is an inadequate statement of the multifaceted role that language may have on children's learning. We have been carried about as far as we can go at the present time with the verbal mediation hypothesis. It was effective in stimulating a great deal of research, but now that fairly definitive studies have been done we find that we must know more about developmental changes in the relations between language and action before we can go much further in understanding the role of instructions or verbalizations in children's learning.

What does all this add up to? It means that we have to be increasingly attentive to the characteristics of the subject with whom we are working--the child, with his abilities and limitations, his preferences and his expectations,

his values and his goals, his words and his actions. While it is important to be aware of developments in the general field oflearning, we will find answers to our questions by being more concerned with the characteristics of the organism we have chosen to study. These characteristics seem to be important because they determine how and what the child will learn.

We are designing sound experiments, conducting them in a rigorous fashion, and subjecting them to appropriate analyses. The studies being done in the mid-sixties are among the most sophisticated studies of children's learning that have been done. But we cannot continue to use hypotheses, concepts, and theories that were not developed with children if we are to continue to make progress in the study of children's learning. While we are grateful for what we have gained by looking at studies with animals and human adults, we must follow our own paths. This is the crossroads we have reached. If we are to have a significant impact on teaching practices and on the understanding of the development of learning abilities, we will have to spend a greater amount of effort in designing studies that tap the unique characteristics of the child. I have used only four examples of such characteristics, drawn from individual differences, sex differences, incentives, and the effects of instructions. One could cite many others, such as the development of strategies, sensory modalities in learning, imitative learning, and personality factors in learning. So much information is needed that the efforts of both educational and developmental psychologists are necessary for rapid advances. If research continues at the pace of the sixties or accelerates, we should, in another decade or so, be able to make a great many firm generalizations about the basic processes in children's learning, with correspondingly confident applications.

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